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*THEORY AND BEHAVIOR ANALYSIS:
COMMENTARY ON DONAHOE, PALMER, AND BURGOS*

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The target article raises a number of interesting issues and comes to several conclusions with which most can readily agree. Operant and Pavlovian conditioning are measured with different procedures but are not completely different processes; Skinner's goal of explanation at the level of moment-by-moment behavior is a desirable one; and neurophysiology does not invalidate behavioral laws. I can add only a couple of comments.

First, although Skinner often urged moment-by-moment analysis ("Farewell my lovely!" and so forth), his consistent antagonism to real theory inhibited theories at that level. Because only "laws" (like Weber's law) seemed to be acceptable in behavior analysis, theory has for years been stuck at the level of molar laws. This development was not, as Skinner complained, a reaction against his ideas, but was in fact the only path he left open. After all, if all theory that "appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner, 1950, p. 193) is prohibited, but we want to explain things anyway, then molar laws are all that is left. Skinner

was not worried by the fact that his prescription would have ruled out most of the great theoretical developments in physics and biology, from the atomic theory and the theory of the circulation of the blood through genetics and the wave theory of light. Almost every important theoretical advance in science has postulated "events taking place somewhere else [or] at some other level of observation." Donahoe et al. are quite right to insist on the necessity for real-time theory, but are wrong to credit Skinner with sympathetic anticipation of their proposal. Far from promoting the solution, Skinner's stance on this issue was part of the problem.

My second comment concerns the main point of the target article: whether reinforcement acts to strengthen responding or stimulus-response connections. This seems to be a straightforward empirical issue: Is operant learning context dependent or not? In other words, after training does responding decrease when the context is changed, or not? Is there a generalization gradient? The answer obviously is, "Almost always." With very few exceptions, operant learning in mammals and birds is subject to stimulus generalization decrement. Therefore, reinforcement must act not just on the response but also on its connection with context. Nevertheless, not all organisms show context dependence. The

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operant-like behavior of orienting microorganisms, for example, doesn't seem to be under stimulus control (Staddon, 1983), and it is perfectly possible to design an operant mechanism that is context independent (Staddon & Zhang, 1991). So the question is certainly worth asking.

Finally a comment on the authors' question "Are neural networks capable of simulating the effects of nondifferential as well as differential operant contingencies?" (p. 202). As McCulloch and Pitts (1943/1965) showed many years ago, even very simple neural networks are general computing devices of the same order as the Turing machine, and hence are capable of simulating any well-defined process. The scientific issue, therefore, is not whether a given data set can be simulated by a neural network (it can), but whether a given simulation is the simplest and best—truest—model for that data set. What is

"true"? Francis Bacon quotes Jestling Pilate asking that question in another context, but Pilate "stayed not for an answer," perhaps because it is not a question that has (as the mathematicians say) a "closed-form solution."

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BIOLOGICAL SUBSTRATES OF OPERANT CONDITIONING AND THE OPERANT-RESPONDENT DISTINCTION

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At the outset I should identify myself as a fellow advocate of the views of Donahoe and his colleagues—as someone who shares their selectionistic approach to behavior, admires their work, and embraces their positions on

many specific issues. In particular, I enthusiastically endorse the main organizing idea: that complex behavior "is best understood as the cumulative product of the action over time of relatively simple biobehavioral processes, *especially selection by reinforcement*" (p. 193, emphasis mine). And with regard to the important issue of the nature and complexity of the reinforced response, Donahoe et al. and I hold the same minority position. Together we reject the common supposition that the "whole" response (or its complex neural substrate) can be identified as the functional unit of reinforcement. Rather, we assume that the unit of reinforcement is some sort of infinitesimal response element or be-

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